Group Interview 2

VR:…start to appear, and you all started to use it. You've mentioned several meetings – the one that Robin gave the inspirational talk at. Did you use the continuum?

RV: Yeah, I think it came out of the Rockefeller study by Lincoln. He did a number of streams…

VR: Did you coin that term?

RV: I don't know if I coined it. Continuum has been a word used for a long time.

VR: I know the continuity of rivers was discussed by geomorphologists.

WM: I think that Robin used it that first time that we heard his ideas presented. Sort of a continuum of changes.

KC: Because we were all segment people until then.

JS: You guys were into zones.

KC: Yes. Yes.

VR: Typologies.

WM: Yeah. And you know, even James has written recently, and he still hasn't got it. This idea of continuum moved in transition along a gradient is the important thing, rather than the breaks, and the whole European model – it just takes away this idea of transport, for example, being important in moving all the way down the system

KC: I remember thinking at this meeting you talked about, when you started wringing your hands, the idea of the downstream systems are living on the inefficiencies of the upstream systems, "Now there's something nobody's ever mentioned."

RV: And that's where the pollution aspect comes in. Looking at so many streams that were polluted and didn't have the structure of the communities that utilize the energy, as streams become progressively impacted, they lose species and they lose energy downstream.

KC: I remember the other thing from that. I'm not sure if it was that meeting or the one in Idaho, that all these bugs were really evolved to kind of play the game. Play the best stock. You can't predict what the hell is going to happen. Throughout the day it could be high or low, but they had put their money on the best bet.

BC: They made the best they could do with what they were getting, and asked the rest of us for…

KC: That's why they got fooled so badly by all of those dam reversals of flow and release of water during the dry season, holding it back during the wet season. That's how Florida got totally screwed up. You hold the water back when it normally came down, and release it when it was normally not there. No wonder they're confused.

VR (2:50): How did you guys communicate pre-email? Did you ever phone call, or was there fax? I mean…you telephoned each other a lot.

RV: No fax. Fax wasn't around.

KC: Ton of phone calls.

WM: And then, through the mail, any document that needed to be transported.

KC: I mean, all of this would've been spam now.

WM: Extremely low tech. That was why on these very short time frames, the Stroud location close to DC was important.

KC: You just hopped the train, and you hit the funder.

VR (3:40): Let's actually talk about the funding for a minute, and the grant. When you were proposing this, there was both a research component, and a data gathering component. Was the data gathering kind of the preparation for the proposal, kind of like when you had called me, that I mentioned, about Minckley's study? Do you remember how that worked?

KC: You mean when we were trying to get the story together before we…? Yeah. We were all going to present some little compartment of this. We were still segmented.

BC: Yeah, I don't have a big recollection of that incident, to tell you the truth. I just know we were all scrambling for little pieces.

WM: We got the inter-biome comparisons thing, right? We made phone calls and got together each day we could.

BC: name called us a phone call, because I remember that I called to get the stuff, and it was Minkley's. I don't even remember that, but I know we must've been doing a lot of this.

KC: Well, we had methods workshops.

JS: Those came before, but beforehand, there was this series of – starting at Triple A S and the Rockefeller and then Oregon State and then at Gull Lake – I mean, we essentially got that to get together to explore cross-biome on streams. That was an NSF funded

proposal for about 25K. That was really used to move us around, I think move to that final Stroud meeting. That was when we got to explore whether we had something worthwhile to put together. Really, the outcome of that was the big proposal.

KC: That was not methods oriented at all.

JS: No it wasn't

KC: So we had to backfill with all the methods after that. We had a lot of discussion in workshops and stuff to try to figure out what we were going to measure.

BC: We had a PR workshop, and a detritus workshop, both at Stroud. These came after the Pocatello meeting in '75.

KC: When was it we got Callahan into the river with us?

BC: I got that as…that's down the road here. It was the detritus workshop – December '75. That was preceded by the PR in August '75 when, man, we all descended on that Stroud meadow with boxes and…

RV: Chambers. We had respiration chambers and…

KC: Remember how we all had bottles, and you said "Everybody sample"…

WM: No. That's the detritus one. That's the second one. That was a review for our second proposal, right?

JS: And Callahan was there.

WM: Yeah. And Risser, wasn't it?

BC: No. It couldn't have been, because, from the stuff I'm working from, in December '75, we had to standardize, and decide what we were going to use for metabolism.

VR (6:38): This was after the grant was funded?

BC: Yeah. And we had the in-stream boxes, we had the Stroud ones that were in the channel outside, brought it upstream, downstream, oxygenated, and I ended up checking out the pH.

KC: Donna King, was Donna King at that one?

BC: Yeah, Donna King was there, and that was in August of '75, and then we added them up, and decided that the in-stream boxes were the way we were gonna go. In December that year, we met to look at detritus. We tried the nested nets, the boxes, the bomb, literally bigger than legal, and it was at that time that we did the great carbon

sweeping rocks thing, and Callahan was there. He got the only pair of leaky boots in the bunch.

WM: Huh. Why would Blair and Callahan have come?

BC: I have no idea.

WM: We up for review, and we were stranded…"What are we going to do with all this time? Let's go on out, and try this cooling experiment." The two had to come together.

RV: Because we went out to our site on the King Ranch for that big sweep.

KC: Oh, that's where it was. Yeah. Right.

RV: That was after our first year of study.

BC: Well, I can show you the slides that show us trying the nested boxes, the bomb thing, all these. And they're days at the same time as the big carbon experiment.

KC: We might have tested them again.

WM: I don't remember that, but I do remember that they came as part of a review, and we utilized that opportunity to go out into the field, in part because we didn't know how we were going to fill two days with presenting them with one idea…

KC: But every time we got together, Brock would have some gizmo for something.

BC: I dunno. Maybe…

WM: Bob Peterson was with us at that time, too. So he had been hired at Oregon and functioning for some time.

RV: That was probably after our first year of sampling.

BC: We had a workshop to test the devices and what we agreed on after that, was that it didn't matter which of the methods we used as long as we all used the same mesh sizes. We didn't care whether you pumped it through, whether you put the net's anchor, the only key was whether you could know how much water went through, and we used the same mesh sizes.

KC: The purposes of the broom experiments was to see whether, in fact, you could sample the thalweg at one place on the stream. Because we had everybody across the stream with their bottles, and we filtered it all. We waved the filters around, and you could see the…

RV: That might have been in preparation for the Salmon River study.

JS: Yeah, I think it was.

RV: Not the initial selection of methods.

BC: In that case, I've got a mistake here, because I thought…

WM: Well that's why we pushed it a little, because we thought you were fallible, and… (*everybody laughs)*

VR (9:58): Had you guys started any writing that led to the 1980 paper at this point? Was anything written down?

JS: It was primarily from the original proposal.

VR: So the original proposal was what led to the 1980 paper?

KC: That's the short answer to that question.

JS: I mean it really then got tailored for pub, because I mean in my recollection, the storyline was there, and you could understand the storyline from the proposal, but for a journal article, it really had to be edited and picked up.

VR (11:22): So, let's hear the story of the 1980 paper, the classic, three times more cited than the next paper in aquatic ecology.

WM: I think that we have on tape already, perhaps. A lot of this was from the proposal itself. To me, an important requirement of the funding had been that this would be put out for raw peer review. By requiring publication of it before our renewal would come about. So we had this, I think, healthy pressure on us. And we've seen this in other of the NSF's actions subsequently, where early on, the ideas have to be made public in a published form. We were coming up on a deadline to get that out. So, we put together that final document that was initially submitted to American Naturalist. We got a relatively quick response back, because the editor refused to send it out for review, or to have other people consider it. He unilaterally said that it was rejected and being returned, because there was no testable hypothesis there. Soon after that then, almost instantaneously, because we thought the manuscript was in good shape, we flipped it over and submitted it to the Canadian Journal of Fisheries and Aquatic Sciences. They were willing to review it, first of all, they sent it out for review, and the reviews were constructive, encouraging, basically it should be published, but with the normal modifications, and so on. So, it was honed and resubmitted, and I think we could claim that it was at least in press by the time our deadline was up, if not already published.

JS (13:15): There's no question that getting the paper out was important for renewal, because NSF had gone out on a limb to fund it, because the initial reviews for it – it just

came out of the blue for streams – and it didn't get the highest reviews. Some people really liked it, but overall, the average was not a fundable score.

WM: It's certainly straight from Jerry's hand.

JS: Right. And once the paper came out, he had something to justify why he would go out on a limb to get it going.

WM: And then to go back in front of the panel. "Hey look, I took a chance, but here it is, it's out there now, and we can be critiqued by our peers much more profitably."

KC: So then we hired a post-doc who would…once we got it started, we hired a post-doc who really got to work. The idea was that everybody goes to their own site, and does all these techniques, and then the next cycle, we all go to the same place. I can remember the picture in the Salmon. We had how many? Fifteen PR chambers out there. You look out there, and it's just this ocean of chambers with batteries and wires and everything.

VR (14:40): Is that the second grant?

WM: Yeah. I did do a fieldtrip out there. I recall that, where we were standing out on the banks, checking out the river systems, and Hall, I know, was along on that trip.

RV: One thing that might be interjected at this time is the putting together of a data recording sheet for the first year of study. In each biome, the data sheet had to be completed so that there was a number of transports in each category, that was a number for primary production, there was a number for the detrital leaf category. There was some initial opposition to having a uniform data sheet for all three. Keating, who was an important confidante of Bob Peterson's put this sheet together, and then the data could be scanned optimally. Everybody had to have the data sheet set up, and this was very important.

BC: This was Bob's big dealing…

RV: Data management.

BC: And also, it might be good to have on the record that Bob spent part time at Stroud. He stayed there for how long? I think he was at Pocatel for a…I forget his time was divided.

WM: I think he was at least at Stroud for the first year.

RV: Yeah. Maybe even the first year and a half.

BC: And then he went to Pocatello for the last part…

WM: Especially when we were setting up to do the big river project.

VR (16:45): One of the things that after the 1980 paper came out, there was immediate embracing of the idea by the vast number of researchers, but there were two papers that came out, one of which had a really negative tone, by Winterbourn and Rounick – maybe I've got the authors reversed – about New Zealand streams being different. Did you think there was anything constructive in that paper?

WM: I have a couple things. I won't answer that question directly, but the first thing is that it was published in what was a relatively obscure place, so it didn't surface right away. That was sort of....there was some lag time before that really came to fore, although I thought Rounick had been a thorn in people's side from even Oregon days.

JS: Well, I wouldn't call him a thorn. He came out and did his undergraduate thesis for Hampshire College. He and Nancy Grimm were both Hampshire graduates. And so, they came out and did their senior project at Oregon State, and so we knew a lot about Jim Rounick, and Jim Rounick was a tremendous verbal communicator, and couldn't write for diddly, but he had an amazing ability to go and keep talking to someone until he got…like Dave McIntire and others. And then he went with Winterborne. Winterborne really taught him how to write, and then he was all excited about it. He was always challenging you, and that was a plus for us.

KC: He was infected by Dave Rouster while he was at Oregon. They gave him the Dave Rouster injection. He and Nancy both. I'm not sure that they ever really recovered, to tell you the truth.

WM: Well, it seems to me he also had something on about functional feeding groups. He had a hatred for them. He could not think rationally on them. And to me, that paper that he and Winterborne wrote was more fueled by that dislike and disaffection with it then it was anything about the River Continuum. And I think, my personal point of view is, knowing now more about those streams, they're sheet disclimaxes, so in a way, they really are different. And in another way, the water still runs downstream, and if a leaf were there, it would get chewed up by the detritivores.

KC: They also had a meeting in New Zealand. I went in the north island, and I went to the only remaining old grove forest that's in the north island, they have a park, and it's full of everything you'd expect to find in a forested stream. When the Maoris came in, and cut the damn forest down, killed all the flightless birds, and now they're highly revered for that. They changed the landscape of the north island. The south island is a whole different thing. That is a major geomorphic…completely resorted island every year. They are different, but the invertebrate story that's different in the north island was they took the story out early on, but if you go to Curry Park, it's just classic. I mean, it looks just like…all the places in Australia look exactly like that too. I think they're different. No question they're different. But I think they forgot to mention some of the reasons why they're different.

RV: The deforestation in Australia, and the impact of the sheep there is the same. Then there's the meadows, the streams, the reforestation programs in Australia.

VR (20:32): Although Lake did not object to it. Lake's response was not a negative one to the continuum. He wrote a response, I think a response to Winterborne is what it was.

WM: I think Sam had a bigger perspective, and didn't get locked into that. And Winterborne, first of all, by printing it where they did, they thought they could get some mileage out of it, without it being as high profile as it became. I'm having trouble remembering this, but I don't know that it ever bothered us enough that we thought it had to be answered.

JS: Oh, no. We invited him back to Oregon State for a seminar. He came back, spent a couple days, gave a talk, Stan and I were there. I don't know if you were still there.

KC: I was there. Yeah.

JS: And so, he was very excited about what he was doing, and we couldn't get excited about it. You know? "Great job, Jim." It wasn't like it moved us to want to change it, or that he had an alternative, a concept that fit something better. I mean, okay, there's an anomaly here.

KC: Sam Lake and Ian Campbell, and some of the others put a workshop together at Threadbow in the Snowy Mountains. After that, Ian organized a camping trip with Winterborne and a bunch of us. And we went to a lot of streams, and so it was pretty clear that Winterborne had not looked at a lot of streams other than New Zealand, because what we were shown in Australia was some pretty pristine stuff that's still left, and it all fit this. You know, you've got to look at the whole basin perspective. He's a really nice guy. On that trip, everybody'd be talking about this, and I think that if you talked to Winterbourn today, you'd get a whole different story than you get early on. I don't know what role Rounick played. Was he the one that injected the story into that?

JS: I think so. I think he pushed it. And again, Jim's a perfect straw man to look for exceptions or whatnot, and so he was very excited about what he'd found. Probably, like you say, much more so than Winterborne. Although, we had Winterborne out too, because Norm had spent a lot of time with him, but Winterborne didn't have the same passion of the paper that Rounick did.

VR (23:22): There's another paper by Statzler and Higler that took a different point of view. Many of their comments were language. I think it was a different interpretation of…a literal interpretation of words. And then, of course, then they had the hydraulic overlay that they added. I know that he went to Oregon State. What was your feeling about that paper?

WM: I was actually asked…that really stimulated the paper that we followed with, the modifications, the clarifications it really what it was.

VR: And where did that paper appear?

WM: That's in Canadian's journal as well. Because it was appropriate and I think at the time that it was such that that would be the follow-up. That would be where we would go with it. Having been told that we didn't have a hypothesis to test anyway, we wouldn't go back to American Naturalist. We were asked to comment, to write a rebuttal. I think the editors contacted us, and asked us if we wanted to write a rebuttal. And basically, we had a lot of iron in the fire, and were pretty busy, and I didn't want to spend the rest of my life being a defender of the River Continuum concept. I thought it was out there, and people could interpret it. I also thought strongly that there were some language problems at the time. I think we were scheduled, maybe you initially and then we decided to do a joint presentation at a NABS meeting in…Duluth? Was it in Duluth or up in that…no, it wasn't there.

KC: Lacrosse.

WM: Yeah. Lacrosse. So, I basically, Ken was tied up with other things, and I gave that presentation, and it was the core of what we went to Canadian Journal with, but it was designed to and answer those valid criticisms that Statzner and Higler had. So that was to be our answer, without having to go to every damn brush fire and trying to put it out. We tried to sort of say "This is our thinking now, this is our update of it, and this addresses some of those issues, maybe the most important ones that were raised in that paper."

VR (25:38): This is where the scale would slide? That was a great addition.

JS: So, was Webster one of the original reviewers of the proposal, or the paper? Because he signed his review, and he was very critical. I remember we brought him out to Oregon State, and I think we were there to essentially get...We brought him on a review to get his ideas, and man he didn't change. He was just locked in as a critic. Again, it's one of these things, and Statzner the same thing. I don't know if Statzner felt as deep a philosophical difference with continuum ideas as Webster did, but Webster became a big proponent and essentially…there was an amazing ability among the stream community, and it may still be there, not to get personal. So you could just be terribly vicious in the intellectual discussion, then we'd break and go for a beer, and we talk about things and find in common. Yeah, we could be harsh, but it never broke away, and we continued to use Webster as a sounding board.

RV: He went on the site review.

JS: That's right. That was what it was.

KC: It was Fisher and Webster who came.

VR (27:15): And Jim, I remember you and Peterson went and visited Statzner in Karlsruhe You said the same thing, that you had these long, long discussions, but then you drank wine all night.

JS: Oh yeah. Right. I mean, he could disagree...Well, I think he could sort the person from the ideas, and he didn't have to personalize the idea for it.

WM: I think it was at Calgary where I got on the bus, and Statzner comes up from the back and sits down. "I've been wanting to talk to you about this." And he started wagging away at me. When we got off the bus, we started drinking beer, and next thing I know, he's on a bull.

VR: I think that's the great thing about field biology. So, can we talk a little bit more about the research? Robin had just mentioned this data sheet that enabled you to do that. What are some of the other innovative things that you think enabled the research.

KC: One thing about that that was so neat was that this was a minimum set of things that everybody had to enter. But you were allowed to do anything else you wanted, that you were willing to support, and so there was a lot of insularly stuff that came in, but everybody had the same set. Otherwise, you could never have made a good bio paper.

RV: It forced people to be on time, do the work that had to be done.

WM: Our side-story was we developed the peanut butter and cracker idea, where we would put peanut butter on the leaves, and try to run and… (*everyone laughs)*

KC: I've got a picture of one of us, maybe me and Ellsworth…

WM: That's the turnaround. It's only fair that he could help you with that.

VR (29:20): How about Bob Peterson? Can we talk a little about him, because he's the one person that we can't hear his story. What do you think his contribution was?

RV: Oh, he was critical. It was a critical position, and he had enough muscle to get his job done, and that was to make sure we got our job done, and collected the data. We did have, Cindy Staub was the data manager at Stroud, and she was very good too at putting together the data slips.

JS: But the analysis, just the analysis and all of this was Peterson straight.

WM: On a couple of these, Bob came with the data. It was all laid out, he could manipulate it, he could get the means and variances out of it and so on, we could talk graphing and so on and so forth. He was really critical. And he had the ability to push. Sometimes too much, and he pissed some people off.

JS: Yeah he did!

WM: But he would get it.

KC: Remember the PR workshop where it was a hundred and forty and humid at Straud, and these guys are out all day, it's a twenty-four hour study and everyone's got their own devices, and I remember it was the middle of the day, we were sitting in the stream, just about dead from all of this work, and Donna says "Bob, go get us some ice cream." He said "How can I keep it cold?" And she said "Put it next to your heart." (*everyone laughs)*

BC: I'll throw this in too. When we were up at the ranch, after one of these sessions – I don't know if it was the one with the divers versus the bug-pickers, but I remember sitting on his porch…

VR: This was at Wayne's ranch?

JS: No. This was at a place near Twin Peaks, where we were doing a…

WM: When we were working on the Salmon.

BC: And he was sitting there, and he said "Cush, I tried." That's all he said. "I tried."

JS: He had a tough job. A real tough job.

BC: We're talking about McCullough and Hawkins and Naiman and trying to keep those in different camps. It was really tough.

WM: I really bonded with Bob when he came out to work with us in getting it set up. I loved the guy. I think he was one of the best people I ever worked with. Then when we had to do the writing, he would be there. He would come and spend a week, or whatever it was, have a lot of stuff together, we'd bang out ideas, he'd take a section, I'd take a section, and he was able to pull a lot of information for us.

VR (32:11): Was the colonization paper that you and he did, was that part of the continuum or was that something you did extra?

WM: No, totally different.

VR: That's a great paper. It really is.

WM: It was a lot of fun to do. And that's the nice thing about Bob. He could spin off his own set of really sparky ideas, and get excited, and just take it and run with it. Well, that was one where I'd actually been asked to do this presentation, to write this paper in honor of T.T. Macan in a series of papers that were being presented. I sort of had some of these ideas, I had some data, but it didn't go far enough. Bob and I started talking – I think I might have been on sabbatical leave in Pennsylvania then – Bob came in, we

spent a week walking lanes, talking about it, just sort of banging out ideas, and he brought a lot to that paper. A lot of different ideas.

KC: He was good. I remember when he was at Kellogg, he would come in in the morning, and he'd say "You know what we need to do…" and I knew that meant another six months, but I knew that he'd complete it. His thesis is still one of the best insect ecology things – it's not published – that I've ever seen.

WM: On *Nigronia*?

KC: Nigronia. It's a classic piece of work, it's just never been published.

WM: We called him seventy percent Bob. He'd take a project to seventy percent, and then he'd be diverted for one reason or another.

JS: I can relate to that.

KC: As Robin said, he was really critical I think, because you had guys like George Spengler who were really good at filling in data sheets, but without keeping all of that stuff together, it would have been just a mess.

WM: And his computer expertise at that time was really good. We had large data management, and then stats analysis. Those were just sort of incomprehensible to a lot of us.

KC: Yeah, I know. He just loved his Macintosh.

VR (34:22): One last question about him. Why did he go back to Sweden? Why didn't he stay in the US?

KC: He went, took a post-doc at Umeo?

VR: No, no. After the continuum.

KC: Well, his wife was Swedish, and he met her at Gimeo, and brought her over to the States, and I think his family was the top goal in his life, so he would do whatever it took to do.

BC: You know, I may be wrong, but a recollection to me was that he had a good chance at a job there, better than anything he had here. He had these things, and this offer was better than anything he had. I don't know whether I'm mistaken here, but…

JS: But the issue he had in Sweden was learning Swedish. When he talked to Norwegians and the others, the thing that was holding him from getting a permanent position there was a language thing. Seminars were always in English, and I'm sure by

the time he had spent ten years there, he could understand Swedish, but he wouldn't speak it.

KC: He spoke it in the market, but not at the conferences.

VR (35:55): When we go on from Bob, is there anybody else at Stroud or OSU that you feel really should be singled out from there work, in helping get this continuum to work.

RV: Tom Bott.

JS: Of course, Fred Swanson, and Stan on the primary production.

WM: In the formative stages, not so much Stan. But in the writing and the thinking, Tom had a really big role, and probably Swanson did too.

JS: I know Klug did.

VR: This is on the subsequent papers after 1980?

WM: Well, yeah. Tom…

JS: Tom was there for the original.

WM: Tom provided the inter-biome comparisons data to go, and we had long discussions about it, he did the writing on that aspect of it, so apart from where he took to leave, he was a great team player.

KC: Oh, he sure was.

BC: I still don't want to put Stan too far in the background, because he and Tom were the real drivers at the PR workshop, really thrashing out the data at the end. So, Stan was close then.

KC: Well, you can't forget Jim Brock. He was there all the time. There's nobody in the world that can muster the field effort that Jim Brock had. Three days of Jim Brock would kill all of us.

BC: I think we should document, that he worked like that, and then crashed. We've got a series of slides of him crashed in a sleeping bag in various locations.

KC: He came to Florida, and did this chamber stuff in the Kissimee He brought his mother and she worked. She would change the chambers in the middle of the night. When you're around Brock, you just get sucked in. You work your butt off.

VR (38:19): How about input from other people? Was Fisher still giving you input and

ideas. You mentioned Webster before. Non-OSU or non-Stroud people. Other people. Were they affecting the way the program was going too?

JS: They were on our sounding boards, and while I don't know that we changed what we were doing because of interaction with them, necessarily, later on, they were good gentle critics. Jack Webster was a very harsh critic, but Stuart was a lot more gentle. He could see the storyline, he wanted it developed, and he would get on us with criticism. Again, there's that openness among stream people that when you did go to Fisher's shop, you came away with something. He didn't hold back a lot of things. He didn't think you were going to steal ideas or something. So there was an openness, an exchange that was about. Certainly Fisher and Webster were two that kinda stayed in and pecked away at us, and kept us honest.

WM: Most of these in general, they were ecosystem level people. There weren't that many folks out there with that, and that's still true. The core of the ecosystem research in streams is where these folks are from, you know? So I think that's really critical. Webster was an IBP person as well. He had that kind of a background and orientation, and he gave us access to that whole group of folks. The other person I remember is Wallace. We spun off a lot of things from Bruce, and I don't know whether he was a reviewer or critic in that regard, but we had a lot of interaction that wasn't formalized.

KC: He was a big one.

RV: He was thoroughly enjoyed.

KC: Remember he dropped a boulder on his foot when the topless females came around in a boat? He was holding a rock, these topless females came around, and he dropped the rock right on his foot. He was a real asset.

RV: They definitely didn't go by me. (*everyone laughs)*

KC: Jim Hall hung in there the whole time to, but a whole different kind of thing. Jim is a detail person, but he's such a straight arrow. You don't tell any stories you can't back up. We always told stories we couldn't immediately back up.

WM: Now where did this classic comment come from with this quote of Jim's that "You're trouble is you think the data means something."

JS: It really comes back, as Robin brought up, to the standardized data sheet. It was essential, and yet it was arbitrary, because the systems were different, and some things were reported differently. So the science of streams, and how you understand streams only came through through the data sheet, and our analysis of that, and the cross-comparison. So it short-circuited things. That's what led me to say "If that's all we believe, we're in trouble. Because we're seeing a whole lot more that either we are measuring a different way, or are not measuring, and that's the best we can do for the paper, but I've never been an admirer of straight data that's stands.

WM: That's your genius, too. The ability to look for the big picture, and for the stories. You said it when we started out that the story has always been the foremost part of it, and that's really good. A lot of us took more detailed or…

KC: When we were at the Twin Peaks ranch, when we all got together to work on the same site, the big river site, every night, you and Peterson would have a wrestling match. You'd have a really intense wrestling match, and then you'd both be so tired, you couldn't argue many more.

RV: We also had a number of meetings and trips to decide what major river system we were going to study and look at, and Bob and I went on many of those trips alone because of the cost of bringing everybody east, but we all went to the Alsea That was in Oregon for the initial study, we went to Arkansas, the Buffalo stream, and the Park Service at that time had acquired major segments on the Buffalo.

WM: Who went on that trip?

RV: Bob and I. We met with the Park Service, and they were very excited at us looking at their system. We also looked at the Pine River in north-central Pennsylvania. A number of streams were considered. But, I guess Wayne would have the best…

JS: But those were your data sites on the shallow part.

WM: Although, that was sort of the tossup initially. We looked at several streams. We looked at the upper Snake and the Henry's Fork. We looked at the Bruneau River. I think it was partly through Jim's suggestion – Jim Brock's suggestion – that I took a look at the Salmon River. The more I looked at it, the better it…

BC: We also made the conscious trade-off on the Salmon that despite the veg-heavy upstream data, we were trading off that whole "river of no return" section that we couldn't get to, I remember we talked a lot about that, plus winter access.

WM: But there were two major tributaries, both pristine, that came in in that no return section. We chose it because we knew we had a system that took us up to at least to eighth order, and dumped into the Colombia, much public land, especially National Forest, and a good part, a core of it in wilderness. It was a real strong driver for it.

RV: In the process of trying to select this list of streams to look at, I asked Luna Leopold what he thought would be good choices of river systems that obviously had low impact, and we wanted one that had lots of USGS flow data and information for our calculations of transport, and Luna suggested the Salmon River, because they I think had five gauging stations, and he was encouraging me very much to look at and select the Salmon, but then interestingly enough, after we selected it, I got a lengthy letter from him saying "Why are you taking the Salmon when that is our watershed?" I had to write back to Luna and tell him "Luna, you suggested I take it." We invited Keith…

VR: Keith Slack? Who is this?

RV: No. The guy from Pinedale…

VR: Lyman? Langmine?

RV: Yeah, Langbein. Exactly. So, I'd been out there at Pine Creek at one of their Pinedale stations. They encouraged us to consider the Salmon, but I didn't have any opportunity to interact, so I dunno why at the last minute, we got this big letter.

WM: At least for that upper part, where they did that tremendous geomorphological work, it was a building block for us. Just a lot of stuff laid in place that we didn't have to go and do.

VR (48:03): Did Leopold have much interaction with the program?

RV: He would come to Stroud lab several times a year – one to teach a course at Penn, which was held at the Stroud lab, and then he came as a part of the academy's review process. Hutchinson, Luna, and…who's the limnologist in those days from Wisconson?

KC: Hester?

RV: I dunno. Every time Hester went back, his graduate students were doing what we did.

VR: One other person that Wayne and Robin, you've mentioned as well is Herb Ross. When I first heard this, it caught me completely unaware. Wayne's written a little bit about this, but could you comment about Herb Ross?

RV: Well, I first got involved with Herb Ross with his winter stonefly club, which was in the early sixties, I think, with…

VR: Bill Ricker.

RV: Ricker. And I was in Tennessee at that time, and there's a lot of little mountain cold springs that I was interested in looking at other than the Tennessee River system, so I was following the stoneflies there and in Illinois. Then, very luckily, after he moved to Georgia, Ross would come to Philadelphia several times a year because he was on a something review team or something, but he would stay out at the Stroud lab, so I had a really good opportunity to study with him, and I gained a lot of information from Herb – climatic temperature, distribution , stoneflies and caddis flies. I think getting the ideas from Herb on species evolution and stoneflies was really good. We sort of had the same relationship with Luna. I think that opportunities to interact with these people is crucial in developing a bigger picture.

KC: I thought Alabama was brilliant. They said "Herb, just come and stay, and be here." "What do you want me to do?" "Just be around to interact with people."

VR: Who said that?

KC: Alabama, when he went. Oh, Georgia. Georgia. "Just be here." You know, I thought that was brilliant, because that was exactly what you needed from him. He could just interact with people and talk to people, and that was terrific.

RV: And then I would go down to Georgia, and meet with Herb, spin my ideas about how the temperature may affect the distribution of all kinds of stuff. I used him as a sounding board. And he loved bourbon, and that was terrific! (*everyone laughs)*

VR (51:51): Robin, related to the question I asked Wayne. The thermal equilibrium process. That was separate from the RCC. That was something else you were working on at the same time?

KC: Yeah, that's right.

JS: He had it, but it played. With the mean differences in temperature, they were a lot less in the low order streams, and a lot more in the mid order streams. So I always liked that story, because it did fit the continuum, and that's where you'd often talk about it.

RV: And American Naturalist accepted that one because we had David. (*everyone laughs)*

KC: That was the key, right there.

WM: It's a nice trigger. It fits the signals to the organisms very well, and it was one of those things that had a very definite smooth gradient continuum kind of progression over the length of a river system. Basically, I think we pretty much substantiated on dammed rivers and so on. And I never met Herb Ross. My influence was all through his publications. But I'll tell you, I thought he was absolutely critical. He just helped frame my thinking and bring some of these other ideas into bear, and had a real strong influence in the…

KC: Is this the one where he overlayed each sample – he's got this paper where…

VR: Yeah. Biomes. Yeah.

KC: That's a strong paper.

WM: I mean, his focus was a very narrow one. It had a genius in application much beyond what…

KC: Well, the Michigan and Michigan State guys, we all went to the survey to see Ross and Sanderson and all the guys who were there, so he had a big influence on a lot of us early on from his work in the survey. He was paid by the survey to work on leafhoppers, and he was doing the caddis flies in a room in his basement. His passion for caddis flies was independent of what they were paying him to do. He was interested in evolution.

RV: Like George. Mayflies wasn't his favorite, either.

KC: So these guys have to have passion for it.

VR (54:10): Jim or Bert, did you know Herb Ross at all?

BC: No.

JS: I met him once, but most of my influence on caddis was Glen Wiggins, but he wasn't the same league as

BC: Keying out of insects I accomplished or tried up in Canada was using Ross'…

KC: Yeah, everybody used…

WM: All my work on Morgan's Creek was basically using Ross. Ross and Banks, and…

BC: I sat up there in isolation in Northern Saskatchewan, I keyed them up, and I sent them to Glen for verification, and he was quite happy. That old Ross key, that was it.